but in vegetative characters—such as shape of leaves and arrangement of flowers) were dispersed in broad outline as at present, before present islands were insulated and the present general dispersion of sea and land worked out. The reader will find in the volume a very large amount of information on these subjects compressed into a small space.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Ocean Circulation

HAVING carefully read Mr. Croll's papers in the *Philosophical Magazine* for September and October, I find in them the full confirmation of my statement that his "crucial-test" argument is based on the assumption of an equilibrium between the Equatorial and the North Atlantic columns; the words "to be in equilibrium" or "in order to equilibrium" being used over and over again to fix this as the essential condition of the computation by which the North Atlantic column is made out to be 3½ feet higher than the Equatorial.

No reference to other passages in Mr. Croll's writings can countervail this fact. I pointed out at Bristol the fallacy it involves, which was at once recognised by Sir William Thomson, General Strachey, and other competent authorities. This fallacy becomes obvious in the following parallel case :—

The specific gravity of Ægean water being to that of Black Sea water as (say) 1029 to 1013, a column of Black Sea water 1,029 feet high would be required to balance a column of Ægean water 1,013 feet high ; therefore (on Mr. Croll's assumption of an equilibrium) the level of the Black Sea must be above that of the Ægean in the proportion of 16 feet to 1,013 feet of depth. But that there is *not* an equilibrium between the two columns, is conclusively proved by the deep inflow of Ægean water which always accompanies the surface-outflow of Black Sea water, showing the Ægean column to be the heavier.

Now Mr. Croll has obviously no more right to assume an equilibrium between the North Atlantic and the Equatorial columns, and thereby to deduce from their relative temperatures the higher level of the former, and the consequent impossibility of the thermal circulation as making the poleward upper flow run uphill, than he would have to deduce the excess of level of the Black Sea from its lower salinity, and to assert that an inward underflow of Ægean water is impossible, as tending to raise that level yet higher.

But there is yet another serious error in Mr. Croll's computation, which, even admitting his fundamental assumption, completely invalidates his conclusion. He has entirely omitted the consideration of the *inferior salinity* of the Equatorial column ; which, as it shows itself alike at the surface and at the bottom, may be fairly taken as characterising its entire height. This will make a difference *in the opposite direction* of about one foot in 1,026; sufficient, therefore, if the excess in the North Atlantic column extends to a depth of no more than 600 fathoms, to neutralise the whole 3½ feet of elevation which Mr. Croll deduces from relative temperatures.

Mr. Croll is unable to see what the "viscosity" of water has to do with the question. Just this—that it affects his whole doctrine of "gradients." The nearer water is to a "perfect fluid," the less is the gradient required to give it horizontal motion.

If a viscous fluid be drawn from the bottom of one end, *A*, of a long trough *A—B*, its level at *B* will be lowered more slowly than at *A*, and will remain appreciably higher so long as the outflow continues. But in the case of a "perfect fluid" and a slow outflow, the level will practically fall simultaneously along the whole length of the trough *A—B*. I am quite aware that, *mathematically* speaking, the level must be always lower at *A* than at *B ;* since there can be no movement of any particle from *B* towards *A*, unless room has been previously made for it. But if the time required for the replacement of each particle by the one next adjacent to it be infinitely small, the excess of reduction at *A* will also be infinitely small.

Now, according to the authorities I previously cited, water approaches so nearly to the condition of a "perfect fluid," that very small differences in its density will suffice (if constantly renewed) to maintain a vertical circulation, *without any appreciable*

difference in level. And my position is, that the void created by the slow descent of water chilled by the surface-cold of the Polar area will be so speedily replaced by the inflow of water from the circumpolar area, and this again by inflow from the temperate region, as to produce a continual upper-flow of equatorial water towards the pole, without the gradient which Mr. Croll persistently asserts to be necessary.

I now leave it in the hands, not of Mr. Croll, but of competent authorities in Physics, to decide (1) whether his "crucial test" has the value he himself assigns to it, and (2) whether his doctrine of "gradients" can stand examination by the light now thrown upon it by Mr. Froude's researches. Until some physicist of equal weight with Sir George Airy and Sir William Thomson shall pronounce the doctrine I advocate to be untenable, I shall continue to believe, with Lenz, Arago, and Pouillet, that it is the only one which can account for the phenomena of Deep-sea temperature.

That the temperature of the upper stratum of the ocean is often affected by the Wind-circulation, and is especially thus modified in the North Atlantic, I have repeatedly pointed out. And it is scarcely fair in Mr. Croll, therefore, to continue speaking of the "wind-theory" and the "gravitation-theory" of Ocean Circulation as if they were antagonistic, instead of being not only compatible, but mutually complementary—the wind-circulation being *horizontal*, and the thermal circulation *vertical*.

As, however, Mr. Croll has now advanced so far as to admit that "physicists may differ from him in regard to whether or not the present difference of temperature between the ocean in equatorial and polar regions is sufficient to produce circulation," I am not without hope that in another year or two he may come to accept the Thermal-circulation as a "great fact;" and that he may then make good use of his knowledge and ability in elucidating the shares which are taken by the Wind-circulation and the Thermal-circulation respectively, in the distribution of terrestrial heat.   WILLIAM B. CARPENTER

The Sliding Seat

MOST problems in animal mechanics are of so complicated a character as to be generally referred to direct experiment rather than to mathematical analysis.

In Mr. Wagstaffe's remarks (vol. xii. p. 369) on the analogy which exists between the movements at the sterno-clavicular articulation in rowing, and those permitted by the sliding seat, we have an argument in favour of the latter arrangement. But when the subject is regarded from the point of view assumed by a practical oarsman, the question of actual advantage still remains unanswered.

There are certain preliminaries which must be considered before we can commence to solve the problem, leading to its subdivision into several distinct problems, some of which will prove interesting to the anatomist, some to the mechanician, some to the physiologist. In the following remarks I shall attempt to indicate the preliminaries referred to.
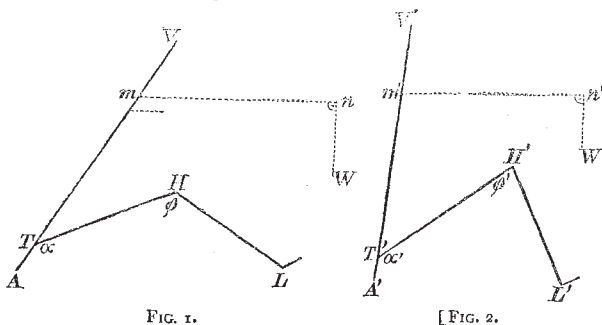
FIG. 1.   [FIG. 2.

Fig. 1 represents the position of the vertebral axis, V A, the thigh, T H, and the leg, H L, when the point A or the seat is fixed.

Fig. 2 exhibits the same parts when A' is movable. In both there is the same position for the outstretched arms, that is, $m\,n = m'\,n'$.

It is clear that in 1 the weight, W, will be raised by such forces as tend to move V A towards the vertical position ; while in 2 the same result is obtained by changing V' A' without alteration of the angle of inclination. We thus see that the angles a and ϕ will vary in definite inverse ratio, while the varia-

tion of ϕ' has almost entirely to be considered in 2. It is this which constitutes the chief difference between the sliding and the fixed seat, and which accounts for the sense of fatigue experienced in the legs in the former system.

If we examine the problems which arise from the consideration of Fig. 1 we shall find that in using the term "fixed seat" we are speaking incorrectly; that is to say, as far as there exists a force to hold A in position we have none but friction; and that practically the position of A with regard to L is determined by muscular action.

Thus in Fig. 2 the seat is really more fixed than in Fig. 1, or there is less muscular action round T' than round T.

The advantages of the system 2 over 1 are however not simply mechanical, but the constancy of the angle a' affords greater space for the respiratory movements, and thus physiologically there is an explanation for the difference in disturbance of circulation and respiration generally experienced when comparing the two systems. R. J. LEE

St. George's Hospital

History of the Numerals

ON reading the letter on the "Origin of the Numerals" (vol. xii. p. 476) I was reminded of some portions of their history which I had before noted down, and which are essential to any consideration of their origin.

The earliest forms which I have seen are those of the Abacus (Jour. Archæol. Assoc., vol. ii.), from which our later forms are mainly, if not entirely, derived. The intermediate forms are to be seen in arithmetical treatises and calendars of the thirteenth to sixteenth century, and on sundry quadrants, &c., of the fourteenth to sixteenth century, in the British Museum.

In the following table the earliest form of each letter and of

each variation is entered, with the corresponding date; the years 1280, 1320, 1420, and 1450 are only approximately stated.

Now, with respect to the primitive forms suggested by Mr. Donnisthorpe, the 2 would seem to have been two strokes at right angles (not parallel), the lower stroke of our form being only a *tail*, like that of many *medial* forms of Hebrew letters. The 3 may have been originally three vertical strokes, which were set horizontal in early times; the flat top, however, does not appear till 1574, and then only in English examples apparently. The 4 of the Abacus seems to have been deserted for cross lines connected, which are always placed diagonal till about 1474, when the first turn to the present position occurs: perhaps four strokes were intended, as we call cross-roads "four roads meet." 5 seems to have been inverted from the Abacus, and then about 1550 the straight tail was curved towards the previous figure, and the head elongated to lead to the next mark. It often occurs as a perfect though very straightened S in the sixteenth century, as it is now made in Belgium and other countries. Its form in 1280 reminds one of the Roman V written as U. 6 in the Abacus consists of six strokes; but this, from their cumbrous collocation, is probably merely a scribe's fancy. 7 has been apparently inverted (like 5) from the Abacus; its transitions are easily traced, but its origin is not so clear; some might see a trace in the Greek Z = 7. 8 has always been very near its present form, and the two squares is an explanation the character of which can only be objected to on the grounds of its inapplicability elsewhere. 9 has always had a straight tail, though it has been inverted since the Abacus form (as 5 and 7 seem changed): its origin might be looked for in the Greek θ possibly, as that letter has varied more in form than any other; or, more likely, in the Arabic Ta, or Tha (= 9), which in the Abacus it closely resembles; and it is even more similar to the Syriac Teth, a twin form to that of the Arabic. Perhaps the ancient Arabic alphabet (in its nearer approximation to its Hebrew- and Samaritan-like original) would show the origin of more of these forms, and even the simple 1 is exactly the Arabic *Elif* = 1, for their alphabetical origin is rendered highly probable from the fact that the numerical systems of the Greeks and of the Semitic nations (from whom our *Arabic* numerals probably came) were in very early times derived from the alphabet; not, like the Egyptian and Roman systems, wholly separate arrangements.

The apparent, though historically untrue, applicability of the line + line origin of all the forms of our numerals, is an interesting example of the fallibility of any theory which only looks to present conditions, apart from past facts and history.

Bromley, Kent W. M. FLINDERS PETRIE

Scarcity of Birds

I QUITE agree with Mr. Barrington, who writes in NATURE (vol. xii. p. 213) concerning the scarcity of birds. I find, by comparing my last year's ornithological diary with the present year's one, that I have only found about three-fourths of the number of Blackbirds' (*Turdus merula*), Thrushes' (*Turdus musicus*), Blue Titmouses' (*Parus cæruleus*), Pied Wagtails' (*Motacilla alba*), Greenfinches' (*Coccothraustes cloris*), Linnets' (*Linota cannabina*) nests that I found last year. The Hirundinidæ have been far less plentiful than usual; but the Goldfinch (*Carduelis elegans*) was the rarest bird here this summer. I did not succeed in finding a single nest, although our yearly average is fifteen. Other birds, as the Charadriidæ and the Mussel Thrush (*Turdus viscivorus*), have been very plentiful, and I found the Mountain Linnet's (*Linota montium*) nest for the first time I have ever met with it on the lowland south of the Humber. Will not the hard frost of last winter account for the scarcity of our native birds in some measure? ADRIAN PEACOCK

Bottesford Manor, Brigg.

OUR ASTRONOMICAL COLUMN

μ CASSIOPEÆ AND VICINITY.—Smyth (Cycle ii. p. 25) has the following remark with respect to stars near μ Cassiopeæ :—"Just 18' south of μ is a star which, though of the 6th magnitude, is not in Piazzi. It is followed nearly on the parallel, about 11^s off, by a 9th magnitude, and both are remarkable from being red, of a decided but not deep tint." There is no star of the 6th magnitude near this position at the present time, nor so far as we know is there any record of such an object having been visible since the epoch of Smyth's observations, 1832·71.